

# JOURNAL OF THE SOCIETY FOR PSYCHICAL RESEARCH

SEPTEMBER 1958

VOL. 39 No. 697

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JOURNAL  
of the  
Society for Psychical Research  
VOLUME 39 No. 697 SEPTEMBER 1958

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MR W. H. SALTER'S RETIREMENT

MEMBERS of the S.P.R. will learn with great regret that Mr W. H. Salter has decided that he must relinquish the office of Honorary Secretary, which he has held for so many years. Though he is, happily, in good health, he has begun to find that travelling almost daily in the winter months between his home at Newport in Essex and the S.P.R. offices is too great a strain to impose upon it.

It will be difficult for most of us to imagine the S.P.R. without Mr Salter as its Honorary Secretary. Presidents come and Presidents go, but we had come to think of our Honorary Secretary as going on for ever. He was first elected to that office in March 1924, and has thus held it continuously for thirty-four years. He had already been Honorary Treasurer for some time before that date, and he continued for some time afterwards to hold that post together with the Honorary Secretaryship. It is no idle compliment, but simply the plain truth, to say that he has devoted his great abilities and the best years of a long life, without any reward except the consciousness of duty faithfully performed, to the service of the Society. In that period he has built up a knowledge of the work and the personalities of those engaged in psychical research, both at home and abroad, which must be quite unique and will for long be irreplaceable.

The post of Honorary Secretary has been by no means a bed of roses. Mr Salter's term of office has covered the years of financial depression and mounting international tension of the 1930's, the second world-war with the bomb attacks on London, and its aftermath of dislocation and inflation. When he might have hoped that the Society was entering upon smoother waters there came the anxiety and upheaval of its enforced removal from the dignified and convenient quarters which it had so long occupied in



31 Tavistock Square. Nor is that the whole story. Psychical researchers, no less than authors, are a *genus irritabile*, and the Society has at times resembled the Primitive Church in the bitterness of its internal feuds, if in no other respect. On such occasions Mr Salter has received, and has returned, some hard knocks; for he is a man who knows his own mind and does not hesitate to give a piece of it to his opponents. But he has always been a clean fighter, and he has always fought for what he honestly believed to be the best interest of the Society and the subject.

It has been said that no man is a hero to his valet; so the loyalty and affection which Mr Salter has evoked in the members of the salaried staff, with whom he has worked year in and year out at the office, is as fine a testimony as could be wished to his qualities. But those who have agreed with his policies, and those who have occasionally disagreed with them, will unite with those who have served under him in wishing him many years of health and happiness. They will extend these good wishes to Mrs Salter, who has done so much for the Society and the subject. And we all hope that both of them will have leisure and strength to contribute by their writings, out of their vast experience, to our knowledge of psychical research and of its history in England during the last fifty years.

C. D. BROAD  
President

## OUR PIONEERS

### II

## FREDERIC W. H. MYERS

BY W. H. SALTER

FREDERIC WILLIAM HENRY MYERS was born at Keswick on 6 February 1843 and thither fifty-eight years later his body was brought from Rome for burial. The Lake country with its old poetic traditions always meant much to him and was the scene of the crisis in his inner life. At the age of seventeen he went up to Trinity, Cambridge. This was doubtless, as he says, 'much too early' from the academical point of view, but had he gone up at a more usual age he would not have been able to enjoy the tuition in Classics of Henry Sidgwick, which led to a lifelong friendship between them, and to the foundation in 1882 of the S.P.R. with Sidgwick as its first President.

Before that date however much was to happen in the development of Myers's mind and character, and in the early stages of organized psychical study. Of his own development he has left an interesting and eloquent record in the 'Fragments of Inner Life' written in 1893, a large part of which was published three years after his death in *Fragments of Prose and Poetry*.

On account of their references to people still living, some passages were omitted, including his account of the great crisis of his life. This, and the 'communications' received after his death and apparently referring to it, deserve fuller treatment than could be given in an article in the *Journal*. I hope to deal with them in a forthcoming Part of our *Proceedings*. In the present article I shall make use of Myers's own account of the progress of his inner life.

From early boyhood he had been a devotee of classical literature, and 'from the age of sixteen to twenty-three', he says, 'there was no influence in my life comparable to *Hellenism* in the fullest sense of that word . . . the classics were but intensifications of my own being. They drew from me and fostered evil as well as good : they might add imaginative impulse and detachment from sordid interests,' but they imposed no moral check on his proud and sensual nature. Though later 'by other pathways' he was led to a creed not unlike that of Plato and Virgil, he felt the need at that time of a different kind of ideal and discipline.

This for a time he found in the 'heroic Christianity' of the philan-



thropic Josephine Butler, who made him realise 'in its emotional fulness' the faith in which he had been brought up and which he had never actually rejected. 'Its drawback was the growing sense of unreality and insufficiency ; the need of an inward make-believe.' Gradual disillusion came to him sadly 'from increased knowledge of history and science, from a wider outlook on the world'. It is to be traced in *The Implicit Promise of Immortality* and other verses written between 1869 and 1873, both being years of immense significance for him.

There succeeded a period of 'agnosticism or virtual materialism', the effect of which on him he regarded as wholly evil. 'During this phase only can I remember anything of deadness and bitterness—of scorn of human life, of anger at destiny, of cynical preference of the pleasures of the passing hour.' In 1869, 'in a starlight walk I shall not forget,' he asked Sidgwick, as his only hope in his perplexities, whether he thought that observable psychic phenomena might solve the riddle of the Universe when other methods had failed, and received his cautious encouragement. This led to an informal association between them with a common fund for the investigation of these phenomena. With the adhesion a few years later of Edmund Gurney and Mrs Sidgwick there came into being the most capable and determined group that had ever attempted research of this kind.

It came as an entirely new experience to Myers when in 1873 he fell deeply in love with a woman with whom marriage was impossible and an irregular union unthinkable. Her sudden death three years later was a shattering blow, but he 'felt with Plato that Love was an inlet into the spiritual world : that it is out of proportion to earthly existence'. From then on he was a changed man, with a faith that satisfied his emotions, his intelligence and his moral sense in a way that he had not found possible in any of the phases of belief through which he had previously passed.

Before however his faith was thus assured, he and Sidgwick had begun their researches. 'I had at first', he says, 'great repugnance to studying the phenomena alleged by the Spiritualists ; to re-entering by the scullery window the heavenly mansion out of which I had been kicked through the front door.' Receptive as he showed himself of unorthodox opinions, he was socially squeamish and exclusive : he needed in William James's phrase, to be 'unclubbed'. The personal second-rateness of many of the mediums with whom he sat perhaps discouraged him as much as the scanty evidence for the genuineness of their phenomena, and may have led him to accept too uncritically those of Stainton Moses, who was at least a man of considerable learning and obvious sincerity.

His meeting with Stainton Moses in 1874 had important results, as through it the academical group of the Sidgwicks, Gurney and himself made contact with several educated men who were much more disposed to accept the phenomena of Spiritualism than they themselves were. When therefore, on the initiative of Barrett, our Society was founded, it included from the beginning, as it has always done since, all shades of belief and unbelief compatible with the view that psychic occurrences deserved and needed systematic enquiry. For this inclusiveness the greatest share of credit falls to Myers.

To this common enterprise all brought the 'solid passionate determination' of which Sidgwick wrote, he and his wife despite the appearance they gave of calm impassivity no less than the high-strung Gurney and the emotional Myers. But each was able to make an individual contribution. Sidgwick was far ahead of the others both in fulness of philosophical equipment and in academic prestige : Gurney showed the greatest skill of the four in experimentation : he and Mrs Sidgwick were better judges of fact than Myers. But Myers had an exceptional gift of rapidly grasping from his omnivorous reading the essential points : Gurney said 'While I am reading a book Myers is mastering a literature'. He had also a remarkable flair for noting the first emergence of ideas destined to attain great significance. A well-known instance is his appreciation in *Human Personality* of Freud, whose name was at the time unknown in this country except to a handful of specialists.

He felt with unusual urgency the desire shared by most thinking men to correlate all the experiences of life, whether derived from their work, their friendships or the books they have read. This is clear from his writings on psychical research and from his published Essays. For him Plato and Virgil, contemporary poets like Tennyson and Rossetti, the scientific, philosophic and political ideas of the time, had somehow to fit into a scheme that would include the facts and theories of psychical research. 'Modern Poets and the Cosmic Law' is the title of one of his essays, and the frequency with which the words 'Cosmos' and 'Cosmic' appear in the most vital parts of *Human Personality* (pp. 112, 115, 119 of Vol. I and pp. 291, 292 of Vol. II) show how central to his thought was the idea of integration in the widest meaning of the word. 'His literary sense', wrote Walter Leaf in his Obituary notice, 'was almost abnormally acute ; but his criticism always leads up to one great question. . . . What attitude does the poet, the historian, the statesman take towards the great riddle of life? What sense has he of the interaction of the world unseen in the things of this life?' Characteristic of his ethical sense is his choice of a line from Homer



which he wished to be engraved on a memorial tablet, 'Striving', as he renders it, 'to save my own soul, and my comrades' homeward way.'

Myers described himself, very fairly, as 'a minor poet and amateur *savant*', a curious combination but one that could not have been bettered for the part he had to play as the principal theorist of psychical research in its early years and at the same time its principal exponent. The traditional fallacious view of psychical occurrences had, with the help of imaginative authors, attached to itself a great deal of popular emotion. The best way of displacing it by a more reasoned view would be for that view to be presented imaginatively and in some degree emotionally. William James found the style of *Human Personality* too lyrical, but if Myers's intention was not only to instruct, but to persuade and stimulate the reader, and particularly to relate psychical research, as he viewed it, with the main stream of human thought, there was much to be said for frequent quotations from the poets and poetic authors of the past, through whom that thought has found its most effective expression, and for introducing them into a context itself ornate.

The development of his character and ideas he put into verse which his contemporaries criticized as too florid and too fluent, and the present generation, so far as I can judge, finds unreadable. Leaf, however, calls attention to the excellence of the short translations from Virgil in his essay on that poet. And some of his other verse gains a great deal in impressiveness from knowledge of the events in his life which inspired it. He could speak eloquently and wittily on all aspects of psychical research. During his life the Society never ran short of good discussion meetings. If Myers had no paper to contribute, without notes and with little preparation he could be trusted to give an interesting account of current enquiries. A sociable man, he talked psychical research to everyone he met, and in this and other ways became the best publicist the Society and its work have ever enjoyed. He succeeded Edmund Gurney, who had died the previous year, as Honorary Secretary, an office he held until his election as President in 1900. For most of the time Podmore was his colleague, and, later, Piddington. He died during his Presidential year on 17 January 1901.

Myers's first work of importance, after the foundation of the S.P.R., was his collaboration with Gurney and Podmore in *Phantasms of the Living*. His share in the work was secondary to that of Gurney, from whom he differed on one point of importance that still remains a crux, the nature of 'collective' apparitions. It led him to contribute a long note (Vol. II, pp. 277-316) 'On a suggested mode of psychical interaction', a subject to which he returns



in Chapter VI of *Human Personality*. While firmly rejecting the cruder concepts of the material objectivity of 'collective', as well as other phantasms, he puts forward a complicated argument involving clairvoyant *invasion* by the percipient. It is conveniently summarized by William James (*Proc.* XVIII, 28) as, 'the subliminal has relations with space as well as with other minds.'

None of the early psychical researchers were likely to overlook the problem of the relation of the psychic faculties to space and matter. To Myers a solution of it seemed of special urgency, and this feeling was doubtless one of the reasons why he was more ready than most of his colleagues to accept 'physical phenomena'. They figure prominently in the remarkable 'Scheme of Vital Faculty' which is one of the Appendices to Chapter IX of *Human Personality* (Vol. II, pp. 505-54).

Myers pursued all his investigations as objectively as he could, but always with a hope that they would provide cogent evidence for survival. For long this hope was entirely frustrated, receiving no support except from a few cases of 'phantasms of the dead' of doubtful value (see *Proc.* V and VI). With the rise of Mrs Piper the position changed. He was a member of the Committee which reported on her first visit to England in 1889 (*Proc.* VI). In common with James and Hodgson, who investigated her in America, and Lodge and Leaf, who took part in the English sittings, he was satisfied that she showed genuine paranormal powers. But was there evidence of survival? On this point leading members of the Society were divided in opinion. Myers, after her return to America, received through her 'communications' relating to very private matters that greatly impressed him. Together with 'communications' on the matter which he obtained a few years later through Mrs Thompson, they seemed to provide factual endorsement of the beliefs he had already reached through his emotional experiences and the philosophic teachings of his favourite authors.

His great work, *Human Personality and its Survival of Bodily Death*, was left unfinished at his death. At his request Hodgson and Alice Johnson prepared it for publication two years later. Had time permitted he might have clarified parts of his theory that have been criticized for obscurity, and supported the philosophical argument for survival by a much fuller treatment of the only serious evidence for it then extant, mainly the phenomena of Mrs Piper. There is, for instance, no account in the book of the 'communications' he had himself received through her, nor yet of those he had received through Mrs Thompson, much as he had been impressed by both. Significantly perhaps the title of the book has been shortened in general use to the first two words of it.

For its importance lies in its being the fullest exposition of Myers's very original psychological opinions. Had space permitted, the temptation to quote pages from William James's Obituary (*Proc.* XVII) and his review of the book (*Proc.* XVIII) would have been irresistible. The following short extracts may be permitted :

Through him for the first time psychologists are in possession of their full material, and mental phenomena are set down in an adequate inventory.

For half a century now, psychologists have fully admitted the existence of a subliminal mental region . . . but they have never taken up the question of the content of this region, never sought explicitly to map it out. Myers definitely attacks this problem. . . .

(On his work on hysteria) he . . . will stand as the original announcer of a theory which, in my opinion, makes it an epoch, not only in medical but in psychological science, because it brings in an entirely new conception of our mental possibilities.

A generation later (*Proc.* XLV, 179) T. W. Mitchell wrote of Myers's view on this subject as being 'far in advance of the teaching of English clinicians'. Not bad that for an amateur *savant*! Not only was Myers the first man to attempt to map out the whole subliminal region, he is up to the present the only man to do so, for however intensive the study given to some parts of that region by the schools of psychology that have arisen since his day they have either denied the existence of the paranormal or given it very superficial treatment.

Since Myers's death neither psychology nor psychical research has stood still. In those areas of the subliminal which they have studied psychiatrists have traced motives unthought of in his day, that may have a bearing on the whole field. New types of psychical research have arisen, and the evidence requiring assessment is much greater in quantity and also more varied and complex than was available to him, notably the evidence concerning survival. It would be idle to search *Human Personality* for answers to our current problems. But for anyone who wishes to know what is the essence of psychical research, what questions it seeks to answer, and how it is related to the thought of its time and of the past, the book has no equal.

*An article by Mr Salter on 'The Myers's Posthumous Message' will be printed in Part 187 of Proceedings to be issued in October next. It will be based on certain facts taken from Myers's private papers, previously unpublished, and should be of great interest and value.—ED.*



## THE CHELTENHAM GHOST A REINTERPRETATION OF THE EVIDENCE

BY G. W. LAMBERT, C.B.

THIS case of supposed haunting of a house by a former occupant is one of the most famous in the annals of Psychical Research. The phenomena were most pronounced during the period 1882 to 1886, after which they 'faded out' and were seldom heard of again. For about sixty years the case was referred to as 'the Morton Ghost (pseudonym of the family concerned), and the town was referred to as 'C'. By 1948 the need for concealment had passed, and the case was published with actual names and address by the late Abdy Collins (*The Cheltenham Ghost*, B. Abdy Collins, C.I.E., The Psychic Press Ltd, London 1948).

The original investigation of the case was carried out by F. W. H. Myers, and a detailed report by the principal percipient appeared in *Proc. VIII*, pp. 311 et seq, under the title 'The Record of a Haunted House'. In *Human Personality* (Vol. II, pp. 388-396) Myers cited the case at some length, and prefaced it with an appreciation in these terms, 'The following case is in some respects one of the most remarkable and best authenticated instances of "haunting" on record, although, as will be seen, the evidence for the identity of the apparition is inconclusive'. Myers' warning as to the dubious identification of the ghost has not always been heeded, and the following re-examination of the story will show that his caution was justified.

The 'haunted' house was known originally as 'Garden Reach'. It stand in its own grounds, on the south side of Pittville Circus Road, Cheltenham, at the corner of All Saints Road. It was built in 1860, and was inhabited for sixteen years by Mr Henry Swinhoe, a retired Indian official. His first wife died during that period (year unknown), and he married a second wife, who left him before he died. His death took place on 14 July 1876. The second wife, who is believed never to have returned to the house as a widow during her life time, died at Clifton on 23 September 1878.

From 1876 till 1882 the house was only occupied for two short periods, for about six months by a 'Mr L.' and later for about three months by a subsequent owner. In 1882 the house was let to a Captain F. W. Despard, who moved into it with his family in April of that year, and seems to have changed its name to 'Donore' (P.O. Directory, 1883-4). Before arrival they do not appear to have heard any stories about the house being haunted. All we

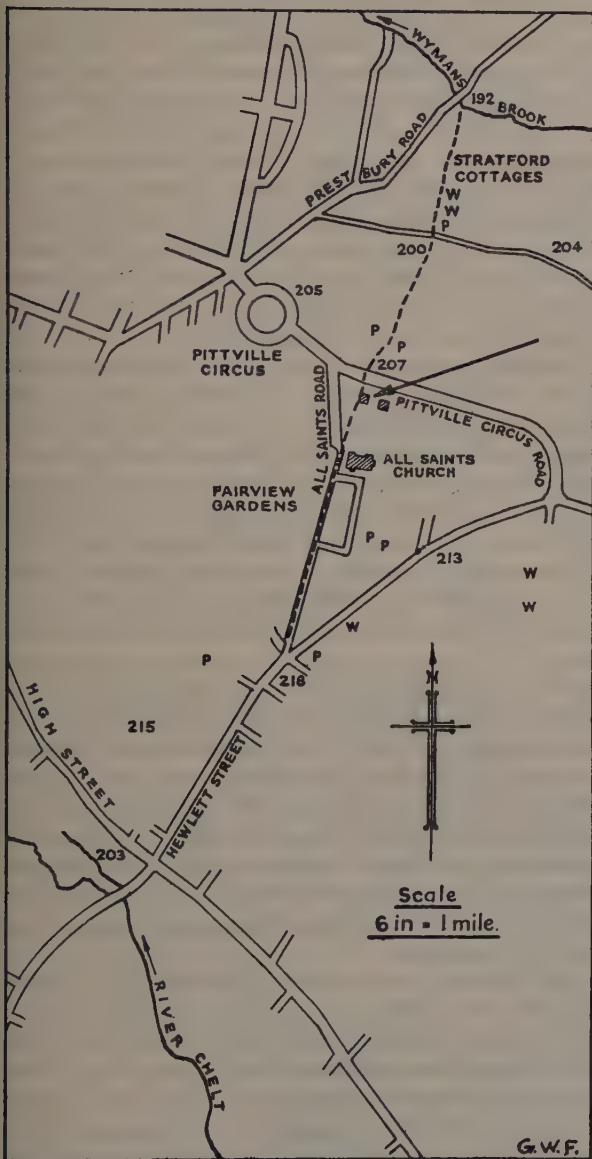
know is that a daughter, Rose Despard, the principal witness, did not *see* anything uncanny until the following June. Her statement does not exclude the possibility that stories of haunting were heard very soon after her arrival, and that unaccountable noises were heard. Be that as it may, the first recorded incident started with the hearing of a noise. On some date in June, Rose Despard, who was in her bedroom at bedtime, 'heard someone at the door'. On opening it she saw no one, but on going along the passage, carrying a candle, she saw the figure of a tall lady, dressed in black, standing at the head of the stairs. The figure started to descend the stairs, but vanished from sight when the piece of candle burnt itself out.

The facts that a noise preceded the appearance of the figure, and that sounds, described as 'footsteps', 'knocks', 'twisting of door handles' and so on, were frequently heard afterwards, suggest very strongly that the primary phenomena were of the 'haunted house' variety, and that the apparition was a secondary result of the mystification caused by the noises. The noises were heard by far more persons than those who saw the ghost, a consideration which points to the noises having been ordinary physically produced sounds, and to the visual phenomena having been mostly or all of a subjective character. Further, the upstairs rooms in the house in which uncanny phenomena were experienced, as shown on the plan on p. 25 of *The Cheltenham Ghost*, were nos 1 and 5 on the first floor, and nos 8 and 12 on the second floor. Nos 8 and 12 are vertically over nos 1 and 5, which suggests that the east end of the house was the part of it most 'affected'. The most probable physical cause of the noises was an underground stream flowing intermittently under the east end of the house. Is there any reason to suppose that such a stream ever existed? And, if it did, is there any reason why it should have practically stopped after 1886? To answer the first question, it is necessary to study the surroundings of the house, as shown on the sketch plan based on the 6-inch O.S. map. Fortunately there was a survey in 1885, and the detail shown may be taken as correct for the period when the phenomena were most pronounced.

That part of Cheltenham in which the house is situated is built on a bed of gravel lying on blue lias (clay). Two streams coming down from the steep slopes of the Cotswolds flow from east to west. One, the Chelt, is about  $\frac{1}{2}$  mile south of the house, and the other smaller stream, Wyman's brook, is about 600 yards to the north of it. The figures on the map show the heights in feet above sea level, and it will be seen that the Chelt at a point south of the house is about 10 ft. higher than Wyman's brook at the corresponding point north of the house. It will also be seen that there is a road



# CITY OF CHELTENHAM (PART OF)



Arrow points to  
'GARDEN REACH'

Dotted line  
presumed course  
of underground  
stream.

P = pump

W = well

Figures show heights  
above sea level.

Scale  
6 in = 1 mile.

## SKETCH PLAN

Taken from O.S. P.47867. 6" Gloucester Sheet XXVI. N.E.  
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branching left from Hewlett Street (centre of map) passing Fairview Gardens and All Saints Church on a straight route, and then veering left along the west side of Garden Reach. An old map of the district, in *A View of Cheltenham Past and Present*, by Henry Davies (Cheltenham 1843), shows that road, now called All Saints Road, as a narrow lane or path, which took a sharp turn to the left at the south end of the plot on which Garden Reach was later built, and then, a few yards further on, turned again to the right along a fence bounding the plot on the west. The ground falls from Hewlett Street (218·1 feet above sea level) to All Saints Church (212·7) and Garden Reach (207·8). It is probable that the line of the road from the junction with Hewlett St to the Church marks that of an old brook or ditch which did not turn left with the footpath as described above, but continued straight on towards Wyman's brook into which it drained. A stream following that course would have passed very close to or under Garden Reach house. The course of the stream lower down between Pitville Circus Road and the point, close to the Prestbury Road bridge, where it joined Wyman's Brook, is perhaps indicated by a series of pumps (P) and wells (W) extending from the gardens of the houses opposite Garden Reach, on the north side of Pitville Circus Road, past Stratford Cottages down to Wyman's Brook (see Fig. 1). There is no other similar line of pumps and wells in the immediate neighbourhood.

It is to be assumed that when the Chelt rose very high in times of flood, water from it broke northwards, and increased the subterranean flow along the line of All Saints Road towards Wyman's Brook. The author of *A View of Cheltenham*, cited above, says (p. 26) that in former times the Chelt dispersed itself along several courses and, when swollen by rains, frequently flowed down High Street. It is a reasonable inference then, that the Chelt was the cause of the noises at Garden Reach, creating disturbance by excessive flooding along a small subterranean stream that passed under the east end of the house.

That theory also provides a very convincing answer to the second question, namely, why the noises practically stopped in 1886, a mystery which has baffled earlier attempts to solve it. The catchment area of the headwaters of the Chelt is the Dowdeswell Valley in the Cotswolds above Cheltenham. Until 1886 Floods swept down the Chelt unchecked except by mill dams, (which must have aggravated the mischief locally), but in that year the Dowdeswell reservoirs, which impounded the headwaters of the Chelt, were opened (G.S. Memoir, *Wells and Springs of Gloucestershire*, H.M.S.O., 1930). The reservoirs took three years to con-



struct, and the opening ceremony took place on 19 October 1886 (*Cheltenham Examiner*, 20 October 1886). The dam across the valley enabled the head waters of the Chelt to be controlled to a much greater extent than formerly, and that presumably diminished the flooding of the channels by which storm water broke north from its normal bed.

A feature of the case which at first sight is puzzling on the 'underground water' hypothesis is the fact that during 1884 and 1885, the years of greatest disturbance, the mischief was at its worst during the summer months (July, August and September), whereas disturbances of this character are usually most frequent in the winter months. The explanation is doubtless to be found in the fact that rain falling on the Cotswold Hills above Cheltenham, owing to the dip of the rocks, drains for the most part south east into the Thames, and not westwards towards the town of Cheltenham; see, e.g. p. 37 of the above cited G.S. Memoir, where the writer says, 'In consequence of the inclination of the rocks being at a greater angle than the slope of the country, the Great Oolite dips down to the south east . . . water absorbed by the Great Oolite to the north west and north gravitates underground to the south east. . . . The Windrush with its tributaries, drains the greater part of the north Cotswolds. . . .' (The River Windrush is a tributary of the Thames.) If, however, owing to the dryness of the surface after a period of hot weather, rain falling on the western slopes could not penetrate down to the levels which drain to the south east, it ran on the surface down the Chelt and similar small watercourses draining westwards to the Severn, often causing sudden and violent floods. In this case one cannot expect to find very close daily correlations between rainfall records made in the town and the noises at Garden Reach, because local thunder showers over the Dowdeswell Valley would not necessarily have passed over the town. But that there was some correlation with rainfall is suggested by the following data, of which those relating to rainy days are recorded in the contemporary issues of the *Cheltenham Examiner* :

<i>Year</i>	<i>Months in which 2 or more incidents recorded</i>	<i>No. of rainy days in month</i>
1884	July	25
	August	12
1885	July	4
	August	12
	September	16
	271	

In 1886 the disturbances were so few and slight that Captain Despard was much less anxious about the reputation of his house, and in 1887, the Jubilee year with its long-remembered exceptionally fine summer, there appear to have been no incidents between February and November.

If one looks at the house today, one finds no obvious evidence of activity by a former underground stream. The walls show no visible signs of subsidence. At the back there are two top storey windows with tilted lintels, but in a building almost 100 years old the effect is not one which can with certainty be attributed to subsidence. A very intermittent stream, with a slight fall, which practically ceased in 1886, would not necessarily have caused any noticeable subsidence, but it might have caused the rather slight noises complained of by the Despard family, at times when it was active.

It is perhaps significant that a rainwater sewer runs from a point about 200 feet south of All Saints Church northwards to a right-angled bend into Selkirk Street, which is a turning to the left off All Saints Road just south of Pitville Circus Road (not shown on Sketch Plan). The first part of its course follows pretty closely the dotted line on the Plan, and it may well represent part of the old natural drainage channel which probably followed that line northwards to Wyman's Brook.

The forgoing circumstantial evidence is inconclusive, but leaves open the possibility that the noises at Garden Reach were due to a natural cause. So long as that possibility remains, it is not safe to assume a paranormal cause to account for them. What then are we to think of the ghost, which was much the most striking and interesting phenomenon in the record?

Was it the apparition of the unhappy second Mrs Swinhoe, who died at Clifton on 23 September 1878? No attempt seems to have been made to discover whether Mrs Swinhoe was tall, like the ghost, or whether she ever wore widow's weeds after her husband's death, which took place some months after she had left him for good on account of his drinking habits. The identification of the ghost was arrived at by Rose Despard on the grounds (a) that it must have been some former widowed occupant of the house, and (b) that there was no other widow since 1860, when the house was built, who it could have been. Assumption (a) is, of course fallacious, and (b), therefore, does not necessarily follow. It is perhaps worth noting that the ghost showed no interest in the small front sitting-room, under the floor-boards of which Henry Swinhoe hid the jewels of his first wife (C.G., p. 89). The identification of the ghost with Imogen Swinhoe was so firmly established



in the minds of the Despard family that the foregoing fact caused surprise, rather than doubts as to the correctness of the identification.

In the absence of any real evidence of that identification, we must examine this case in the light of others of a like kind. There is no doubt that phenomena, in the nature of noises and movements which are believed to be 'ghostly', can so work on the minds of witnesses that some individuals begin to 'see things'. What is still more remarkable is that the things seen, at any rate within the same general social environment, tend to resemble one another rather closely, even where there has been no opportunity for direct suggestion from one individual to another. For instance, noises in a house interpreted as *slow* 'footsteps' seem to suggest a 'human' agent who is seen (by some and not by others) as a *tall* female figure dressed in dark (grey or black) clothes with a full skirt, with her head covered by a hood or shawl. This sketchy 'ghost' is identified sometimes as a nun, (cf. the Ballechin House case), sometimes as an old woman, and sometimes (as in this case) as a widow. If the ghost continues to be seen on successive occasions by different members of a household, it tends to become systematized and to acquire an agreed appearance, as it is described in gradually increasing detail by various witnesses to one another. This process of 'build up' is, of course, quite unconscious. Similarly, a 'swishing sound' suggests a woman walking by in a silk dress. On the other hand, an almost silent walker, as in the case of the Cheltenham Ghost, suggests a woollen dress. ('The figure was that of a tall lady, dressed in black of a soft woollen material, judging from the slight sound in moving'—Rose Despard, *C.G.*, p. 91.) Out of doors repetitive 'padding' sounds, probably caused by wind and water, seem to build up ghosts of 'black dogs', the size of the dog depending on the tempo of the sounds. When it is slow, the ghostly dogs, as seen, may develop legs as long as those of a calf. (For stories of dog 'ghosts', see *The Ghost Book*, A. A. Macgregor, Robert, Hale, 1955 ; pp. 63–81.) In short, the 'producer', who stages these strange visual effects, clearly has his own logic and powers of inference, the exercise of which is hardly realized by the conscious self of the person who sees them. There is a very large field for research into the ways in which, in cases of this kind, sounds are translated into visual effects. Systematic information on the subject would be a valuable instrument in the hands of the investigator of spontaneous cases. Visual phenomena are more likely to be recorded and reported than somewhat vague and inarticulate sounds, and in cases where the former only are reported, it may be found, on further inquiry, that sounds were

first heard, in which event it is unlikely that the visionary material conveyed any paranormally acquired information.

There does not seem to be any difference in the manner in which apparitions due to suggestion are presented to the percipient, compared with veridical hallucinations. Both kinds seem to use the same 'machinery', and to show a good many similar characteristics. For instance, in accounts of apparitions it is fairly often observed that the upper part of the figure is more clearly seen than the lower part; or that the vision appeared in the upper half of the normal field of vision. It is therefore interesting to note the following statement by Rose Despard about the Cheltenham Ghost (*C.G.*, p. 103)—'The upper part of the figure always left a more distinct impression than the lower, but that may be partly due to the fact that one naturally looks at people's faces before their feet.'

Secondly, the ghost was not called up by a desire to see it, or by sitting up to watch for it (*loc. cit.*). It seems to have become associated with the hearing of 'footsteps' in the passage outside. Rose Despard says (p. 93): 'During these two years' (i.e. 1882-4) 'the only noises I heard were those of slight pushes against my bedroom door, accompanied by footsteps; and if I looked out on hearing those sounds, I invariably saw the figure.'

The posture of the figure is also curiously like that of an earlier ghost of the same kind, about which Rose Despard may possibly have read at some time or other. The Cheltenham Ghost usually held her right hand up to her face, and her left hand hung down, pointing to the ground. Compare with that the ghost seen by Mr Drury in Willington Mill in 1834. It was 'a female attired in greyish garments, with the head inclining downwards, and one hand pressed upon the chest, as if in pain, and the other, viz. the right hand, extended towards the floor, with the index finger pointing downwards' (*Journal* 5, 350). Apart from the reversed postures of the hands, the two figures are described in very similar terms. The visual phenomenon, whatever its nature may be, is usually poorly defined, and a good deal of detail is often supplied by the witness, and is therefore unreliable as evidence of identity. The Cheltenham Ghost was seen both indoors and out of doors, and is described in terms that differ. Usually it was tall and had a dark headdress; but one witness described it as a little woman wearing a white cap (*C.G.*, p. 256). There is also some evidence, which cannot now be corroborated by any surviving witness, that the ghost was seen not only in the garden of Capt. Despard's house, but also in the garden of the house next door, to the east of Garden Reach. In the early 'eighties' it is said to have been occupied for a time by a Canon Armitage, with whom he stayed,

when he was a small boy, the writer of the letter reproduced by Mr Abdy Collins on pp. 115-16 of his book, there described as 'Mr L.' This individual, who has since died, was born on 17 October 1878, and was therefore 6-8 years old in 1884-6. His recollections are said to have included incidents in the garden of Canon Armitage's house. It may well be that the subterranean flooding which caused the effects mainly in the eastern side of Garden Reach extended under the garden of the house next door as well. It is hardly possible to explain all the appearances of the Cheltenham Ghost in terms of a physical phenomenon, but something of the kind may well have been the trigger which set off a chain of psychological reactions.

As recently as February 1957 strange phenomena were reported from a house in North West London, in which, in addition to the usual physical effects, such as opening of doors, shaking of a bed and so on, experienced by more than one witness, there was also seen, by one witness only, a woman in black on the stairs, strongly reminiscent of the first appearance of the Cheltenham ghost to Rose Despard. (It is extremely unlikely that the witness who saw the apparition had ever heard of the earlier case.) In that instance the physical effects were almost certainly due to an underground stream at the back of the house, shown as a brook on a 6-inch Ordnance Survey Map, based on a Survey of 1881 (Middlesex, Sheet XVI). The course of the stream, now underground, can be traced today by manhole covers along its course. In one place, a short distance above the 'haunted house', it has caused the subsidence of a building which has had to be shored up with timber.

The question whether, in cases of this kind, the ghost is due to secondary elaboration of a primary physical phenomenon which has been misinterpreted, must remain a problem calling for further research. In rare cases where the primary phenomenon, whether auditory or visual, is elaborated by a 'sensitive', the result may be veridical, and disclose information not acquired by ordinary means. But such an event, while proving the ability of the sensitive to acquire information by paranormal means, does not necessarily imply the survival of a dead person whose apparition is seen. For two cases in point, see 'The Ardachie Case', by W. P. W. McEwan (*Journal* 38, pp. 159-72), and the story of the Cottenham House ghost in 'The Answer is not yet' by Eileen Garrett in *Tomorrow* (Autumn issue, 1956 ; Vol. 5, No. 1, p. 9).

### *Conclusion*

The foregoing considerations, while they do not rule out the possibility that an apparition seen in a 'haunted house' case may be



veridical, place a very heavy onus of proof on those who would support such a claim. Usually such an apparition is merely the externalization of a belief held by the percipient, which *seems* to explain noises or events otherwise quite unaccountable. In other words, the physical noises or other events give rise to the ghost, and not vice versa. This way of regarding the facts would explain an anomaly which has puzzled many people on the traditional hypothesis. In cases like that of the Cheltenham Ghost, the figure usually walks silently when seen, but when it is not seen (e.g. when it is supposed to be in the passage outside), it walks with very audible footsteps.

In view of the fact that 'haunted house' cases are relatively quite common, it is advisable, even when an apparition only is reported, to look very closely into its antecedents, before accepting any identification of the figure offered by the percipient. Once an identification has been made, there is a tendency, understandable on the psychological hypothesis, for the figure, if repeatedly seen, to approximate more and more closely to the appearance, or supposed appearance, of the person identified. The widow, first seen as a rather shapeless hooded figure, becomes more like a widow, the nun like a nun, and so on. This kind of 'false verification' of an unsound hypothesis is a constant peril for the psychical researcher, and calls for continual re-examination and crosschecking of assumptions.

An incidental risk, in the case of a false identification, is that it may be very unkind to the memory of the person identified. In the present case the story as related drags into view the names and sad history of Henry Swinhoe and his second wife, whose deaths were hastened by alcoholism. So far from Imogen Swinhoe having had anything to do with the unnerving incidents at Garden Reach, it is surely more likely that the Swinhoes were themselves unnerved by the mysterious happenings, which appear to have started long before the arrival of the Despardes in 1882, and to have accounted for the difficulty in letting the house after 1876, when Henry Swinhoe died. Indeed, it would be more charitable, as well as reasonable, to suppose that the wearing effect of the repeated noises drove the couple to excessive drinking, and that they were thus victims and not causes of the 'haunting'.

I am indebted to the Controller of Her Majesty's Stationery Office for permission to quote from the Official Memoir *Wells and Springs of Gloucestershire*, and to the Director General Ordnance Survey for permission to publish the Sketch Plan taken from the 6-inch Ordnance Survey Map. I am also grateful to the Borough Engineer of Cheltenham, Mr G. Gould Marsland,

M.B.E., B.Sc., M.I.C.E., M.T.P.I., who kindly answered some questions I put to him, and furnished the information about the rainwater sewer mentioned above. He is, of course, in no way responsible for the inferences I have drawn from that information. For the recollections of 'Mr L.' I am indebted to his widow, with whom I was in touch before Mr L. died. I have not been able to verify from a Directory the occupation by Canon Armitage of the house next door to 'Garden Reach'.

For two other cases of haunting, where mysterious noises were discovered to be almost certainly due to underground water, see Chapter IV of *Four Modern Ghosts*, by Eric J. Dingwall and Trevor H. Hall (G. Duckworth & Co. Ltd, 1958).<sup>1</sup> In these two cases ('Ousedale' and Sowerby) unsuspected subterranean water had been 'at work', no doubt intermittently, for over a century, and the effects on the buildings overhead were visible to anyone looking for them, whereas the house 'Garden Reach' was only exposed for about 26 years (assuming that the flow of water practically ceased in 1886). It is a well-known fact that underground water can excavate quite large channels and cavities without noticeable sign on the surface until there is a collapse.

## DICE-CASTING EXPERIMENTS WITH A SINGLE SUBJECT

BY G. W. FISK AND D. J. WEST

*This paper is based on one printed in the Newsletter of the Parapsychology Foundation of New York in November, 1957, and provided a record of research for which the McDougall Award for 1958 was given to the authors. The later experiments were carried out under a grant kindly made by the Foundation. Details of the earlier experiments with Dr Blundun have already appeared in the Journal and this account completes the records.*

THE series of experiments here described represent a continuation of dice-casting trials with the subject Dr J. Blundun. The purpose of the experiments was to try to demonstrate the existence of an effect dubbed, without prejudice, 'psychokinetic' with a particular subject over a long period of time. Most of the work published hitherto concerns group results and neglects the performance of single subjects. These experiments seek to demonstrate the existence of a consistently successful psychokinetic (PK) subject

<sup>1</sup> See p. 288 of this issue for review.—Ed.

analogous to the consistently successful subjects familiar in extra-sensory perception (ESP).

### *Experiment A*

In 1951-2 G. W. Fisk and A. M. J. Mitchell conducted a series of distant ESP/PK trials with ten subjects (see *Journal*, 37, No. 674, March-April 1953). The experiment comprised the cup-casting of a number of dice for a succession of targets, each displayed for twenty-four hours and changed daily at approximately 8 a.m. by G.W.F. at Long Ditton, Surrey. The order of the targets displayed was determined initially by the throw of a die, but afterwards Fisher and Yates's table of random numbers was used. It will be seen that as the selection of the targets was randomized by these methods the face frequencies were approximately but not exactly equalized. That was a weakness in the experiment which, in the subsequent trials, was rectified by using a randomization method (by Latin Squares, etc.) which provided an exact equalization of targets. Thus the absolute  $t$  values<sup>1</sup> quoted for this first experiment (see Appendix I) are subject to a correction but the amount is too small to invalidate the general Probability (P) values as calculated. It must also be noted that only in the first Experiment A could the methods used seem to permit the intrusion of any spurious effects through dice bias or any vagaries in the method of casting, whether the bias be regarded as fixed or slowly changing during the course of the trials.

Of the ten subjects four showed positive deviations of marginal significance from Mean Chance Expectation, but one subject, Dr J. Blundun, was outstanding, and, on the completion of 10,000 trials, had results of the significance of odds of approximately 30,000 to one against obtaining by chance a deviation as large as that produced.

### *Experiment B*

A further 3600 trials were carried out but with a change in procedure. The targets were no longer displayed in G.W.F.'s study but instead, six sealed envelopes containing the six targets, 1 spot to 6 spots, were marked A to F and sent to Dr. B. who was asked to select the order in which they were to be used as targets by means of an alphabetical 6 × 6 Latin Square provided. No special precautions were taken (as were done later in Experiment E) to

<sup>1</sup> The symbol  $t$  represents the ratio (known as the Critical Ratio) between the actual deviation from Mean Chance Expectation (MCE) and the Standard Deviation (SD).  $t = \frac{\text{deviation}}{\text{SD}}$ .



ensure that G.W.F. was unaware of the contents of each envelope. Actually G.W.F. had no record of the envelopes' contents but may have been cognisant of them, at least, subconsciously.

After completing 600 trials on each target the sealed envelopes were opened by D.J.W. to enable the assessment of the scores to be carried out. In this experiment Dr. B. also secured positive results but of marginal significance only. See the summary of all results with Dr B. in Appendix I.

### *Experiment C*

A third experiment was then planned, on the same lines as Experiment B, with the hope of completing a larger number of trials (20,000). Unfortunately after rather more than 5000 trials had been made Dr B. had a severe illness that incapacitated her and the tests had to be abandoned. In the confusion that followed her entry into hospital and the subsequent break-up of her home, the records were lost and detailed analysis was impossible, but for the 5000 trials completed Dr B. again showed positive deviations from mean chance expectation.

The original scoresheets had been posted to G.W.F. in weekly batches, who analysed them and kept a record of the results. But as Dr B. was sufficiently interested and had offered to examine and check G.W.F.'s figures they had been returned to her just before her illness. We may assume that the figures given in Appendix I are adequately correct though the loss of the data has made re-checking impossible.

In these dice experiments two methods were used in assessing the scores, the ordinary Direct Hit (DH)—termed the B method in the original report—and a technique invented by A. M. J. Mitchell which he termed the Die Orientation (DO) method. For full details of this method of differential scoring reference must be made to the issue of the *Journal* mentioned above. A brief explanation is that when a die is cast for a target, the face matching the target will be found in one of six possible positions: horizontal on top (a Direct Hit); vertical (four possible positions), or horizontal and in contact with the surface upon which the die has been cast (a complete miss). This suggests that the amount of 'error' of a cast may be measured by the angular rotation required to bring the matching face to the top horizontal position, i.e. a rotation through either one or two complete right-angles. The three categories are given the scores of 0, 1 and 2 right-angles respectively and, if, after a run, these angular errors are summed and regarded as the score, it will be found that the mean chance expectation (MCE) =  $n$ , where  $n$  is the number of trials, and that

the standard deviation (SD) required to estimate the probability (P), will be in accordance with the formula  $SD = \sqrt{n/3}$ .

It was found with most subjects (although not consistently with all) the significance of the results was increased by using the Die Orientation (DO) method of assessment, and in the present experiment that was also the case. Hence, apart from the theoretical considerations advanced by Mitchell, the Die Orientation (DO) method of scoring may prove to be more sensitive than the ordinary Direct Hit (DH) method.

Dr B.'s illness was prolonged and during the period of convalescence she spent some time abroad. She eventually returned to this country and one is glad to be able to report that by the spring of 1955, her recovery, if not complete, was sufficient for her to undertake some further trials. Accordingly Experiment D was planned by D.J.W. and G.W.F.

### *Experiment D*

This experiment had two main objectives :

- (1) To see if Dr B.'s previously positive results would continue.
- (2) To investigate if a change of experimenters (D.J.W. in lieu of G.W.F.) would affect the scoring rate.

The targets were to be selected and displayed by G.W.F. and D.J.W. acting alternately—each ignorant of the other's targets. Dr B. was not informed of this alternation and would assume that all targets were being displayed by G.W.F. as before.

The targets were equalized and randomized in this experiment by taking the order of the digits 1 to 6 as they occurred in the columns of Fisher and Yates's tables of random numbers. Thus for the first batch of six targets we might take the first horizontal line of the tables which reads :

0 3 4 7 4 3 7 8 6 3 6 9 6 4 7 3 6 6 1 4 6 9 8 6 3 3 7 1 6 2 etc.  
and the order of target presentation would be 3, 4, 6, 1, 2 and finally 5 to secure a random equalization.

The targets were displayed by G.W.F. and D.J.W. alternately in their homes in Ditton Hill and London, N.W. 3, by putting a card depicting the target for the day on desk or mantelpiece and changing daily at 9 a.m. Dr B. was about 200 miles away. G.W.F. had contact with Dr B. by correspondence only. He met her for the first time at the end of this experiment. D.J.W. had no contact with her during the period but had corresponded with her some time previously.

This alternation of experimenters follows up a previous joint experiment of D.J.W. and G.W.F. in which a number of perci-

experiments called 'down-through' sealed packs of clock cards made up alternately by them (*Journal*, 37, No. 677, November–December 1953). In that experiment there were some indications that the subjects on the whole did better on G.W.F.'s targets than on those of D.J.W. but the difference was not sufficiently pronounced to be regarded as established. Moreover one subject, S.M., was a notable exception to the general trend. S.M.'s results far exceeded those of the others and she was equally, and even more, successful with D.J.W. than she was with G.W.F.

In the present experiment targets were displayed on 60 consecutive days (excluding Sundays) 30 days by each experimenter, in alternating periods of six days each. The subject made 48 trials each day in 16 casts of three dice each. Her scoresheet recordings were witnessed by an observer on about one-third of the occasions. One day, the 57th, was missed by her when D.J.W. was controlling the targets. Dr B. returned all scoresheets by post to G.W.F. who assessed his own results and passed on to D.J.W. his own half. The scores were afterwards independently checked by both experimenters. Appendix II gives a summary of the trials and shows in two columns the deviations from mean chance expectation for both Direct Hit (DH) and Die Orientation (DO) scores.

### *Results of Experiment D*

In the first periods Dr B. started well with positive deviations with both experimenters. With G.W.F. she continued with small but positive scores with an upward spurt in the last period. With D.J.W. she tailed off as the trials proceeded with a flop in the last period. With D.J.W. the final score was almost exactly in accord with mean chance expectation. With G.W.F. the total showed a positive result of more or less border-line significance

DH $t=2.47$	$P=0.013$	Odds 73 : 1
DO 2.60	0.009	106 : 1

Is the difference between the two experimenter's results significant? This is explored by a  $2 \times 2$  Chi-square table :

	<i>Hits</i>	<i>Misses</i>	
G.W.F.	278	1162	1440
D.J.W.	229	1163	1392
	507	2325	2832

$$X^2 (1 \text{ d.f.}) = 3.93 \quad P = 0.05$$

The difference between scores on G.W.F. and D.J.W.'s targets



reaches therefore a 95 per cent probability that it is real. But the difference is less impressive when one considers that no strictly consistent trend is apparent. Thus we see by D.J.W.'s figures he has both positive and negative deviations about equally divided over the five periods of six day runs. A significant difference therefore, while likely, has hardly been established. It is more impressive to note that after the lapse of three years Dr B. is still able to score positive results with G.W.F. of at least borderline significance. If the first results were just a flash in the pan the flashes still continued.

### *Experiment E*

In April 1957 a further set of 3600 trials was arranged. The method, with some variation, was similar to that used in Experiment B. Six sealed envelopes were sent to Dr B. containing cards with the six die faces. They were placed in the envelopes by an assistant out of sight of G.W.F. who afterwards shuffled the six envelopes and marked them with the letters A to F. Hence nobody had normally acquired knowledge of the contents. The cards were wrapped in opaque paper inside the envelopes and the sealing was protected by a device invented by G.W.F. involving the use of water and alcohol soluble dye and finger-prints.

Dr B. was instructed to take the envelopes A to F in alphabetical order and make 600 trials on each before proceeding to the next. Two dice were provided and Dr B. made and recorded 50 casts (100 trials) each day during the months of April and May. Regarding the actual method of casting Dr B. followed the same instructions as before. She had to cast the dice onto a cloth-covered table from a large round cocoa or similar tin lined with corrugated cardboard. Any die falling off the table was to be ignored and a fresh cast made. After each cast she was to record the number of spots on the uppermost faces. She was to have a witness whenever possible to check the accuracy of her recording.

In previous experiments Dr B. had thrown three dice in each cast. As she did her own recording, generally without witnesses, it is likely, even with so careful, scientifically trained and conscientious a subject as Dr B., a few recording errors may have slipped in. It was thought that by using only two dice at a time the risk of such errors would be greatly reduced.

After the completion of 3600 trials Dr B. forwarded her 36 daily records and the still unopened envelopes to G.W.F. who passed them to D.J.W. to hold until G.W.F. provided him with a prediction of which die face each envelope contained. G.W.F. then analysed each scoresheet and assessed the deviations from

mean chance expectation, both for Direct Hits (DH) and Die Orientations (DO), for each of the six faces. A summary of the data thus obtained is given in Appendix III.

By observing which die face on each target had made the largest, and second largest, positive deviation from main chance expectation, taking into account both Direct Hits (DH) and Die Orientation (DO) assessments, G.W.F. was able to frame a prediction as to what each envelope A-F contained. On receiving this prediction, D.J.W., having satisfied himself that the envelopes had not been tampered with, broke the seals and revealed their contents. The following table gives the results :

<i>Envelope</i>	<i>Contained</i>	PREDICTION	
		<i>First Choice</i>	<i>Runner-up</i>
A	6	3	6
B	3	3	6
C	5	5	1
D	1	1	4
E	2	4	5
F	4	6	4

It will be seen that in three cases B, C and D the prediction was correct. In two of the remaining cases, A and F, the runners-up were correct. Only in one case, E, was the prediction entirely wrong.

It would not be very meaningful to try to attach a specific probability value to the result of this one prediction. But the outcome was at least suggestive and warrants further trials on the same lines. Inspection of the figures in Appendix III shows that the scatter of Die Orientation (DO) scores on envelope E is notably less than on the other envelopes. This could be an indication that for some reason the effect was not active during the week when envelope E was being used. The experiment might be improved by a rotation of targets *within each* session.

The question raised by this experiment is precisely similar to that raised by 'majority vote' assessments in ESP data (see *Journal S.P.R.*, 1957, 39, 157-162). Can the inaccurate trends of a large mass of data be used as a basis for more accurate prediction of a small number of targets? The present result is consistent with an affirmative answer.

### *Comments and Conclusions*

The results of these five experiments, spread over a period of six years, provide fairly satisfactory statistical confirmation of the

existence of a persistent effect associated with a particular individual. What is the nature of this 'effect'?

One possible but unlikely hypothesis is that only ESP is involved. Assuming that Dr Blundun could gain some awareness of the targets by ESP, would she have to use PK to produce the results? Since she is doing her own casts and her own recording, largely unsupervised, it would be theoretically possible that she could have produced a spurious correspondence with the targets by systematic recording errors—errors of which she might be quite unconscious through some measure of 'disassociation'.

Or, apart from such ESP inspired recording errors, if Dr Blundun can get the targets by ESP, could she not, perhaps unconsciously, evince an unusual degree of *skill* in dice-casting which would result in a statistical excess of hits? Certain 'crap-throwers' in the U.S.A. are reputed to possess such extraordinary skills. But such an hypothesis would seem to require a stretching of the powers of ESP to somewhere near breaking point, besides assuming that Dr Blundun, without any special and prolonged practice, has somehow achieved the skill of a seasoned crap-player, who, in any case, casts the dice from the bare hand and not from a cup.

At face value the results appear to represent a combination of ESP and PK—ESP to find out the concealed targets; PK to guide the dice in the right direction. But, of course, this picture of what happens may not correspond to the actual mechanism of the mysterious correspondences which make up the 'result'. There is some slight suggestion from Experiment D that the experimenter in control of the targets has some role to play in the final outcome.

The success of PK aimed at concealed targets, besides providing a practically easy and useful experimental technique, raises the question whether the knowledge of what the experimenter wants him to aim at is any help to the PK subject. Perhaps PK sometimes works better with stimuli received by ESP than on the basis of a deliberate conscious aim. Comparative tests with open and concealed targets are badly needed.



## APPENDIX I

SUMMARY OF DR. BLUNDUN'S  
FIVE ESP/PK EXPERIMENTS

Exp.	Year	Experi- menter	No. of trials	Dev. from		MCE		<i>t</i>
				DH	DO	DH	DO	
A	1951-2	GWF	10,000	117	221	3.15	3.83	
						P=0.0016	0.00013	
B	1952	GWF	3,600	53	54	2.32	1.56	
						P=0.02	0.12	
C	1953	GWF	5,000	41	96	1.54	1.98	
						P=0.12	0.05	
D	1955	GWF	1,440	35	56	2.47	2.60	
						P=0.013	0.009	
		DJW	1,392	-5	2	—	—	
E	1957	GWF	3,600	48	94	2.14	2.74	
						P=0.032	0.006	
Totals (excluding Expt. A)			15,032	172	303	3.76	4.26	
						P=0.00017	0.000021	

As Experiment A may be regarded as a pilot study from which Dr Blundun was selected, and, as moreover it was the one experiment in which the possible effect of dice bias was not definitely excluded, it has been thought advisable to omit the figures from the pooled results.

Experiments A, B and C were reported in the *S.P.R. Journal* **37**, No. 674, March-April 1953. Experiments D and E have not been reported previously.

## APPENDIX II

EXPERIMENT D				SUMMARY OF SCORES			
<i>Experimenter G.W.F.</i>				<i>Experimenter D.J.W.</i>			
Sheet No.	Target	Dev. from DH	MCE DO	Sheet No.	Target	Dev. from DH	MCE DO
1	5	0	-2	7	5	5	11
2	4	2	3	8	2	0	2
3	6	4	8	9	6	-1	3
4	1	-2	-2	10	3	2	-2
5	3	4	5	11	1	1	-1
6	2	3	1	12	4	-1	-3
		11	13			6	10
13	6	3	9	19	3	2	6
14	5	-1	6	20	6	0	0
15	3	-3	-7	21	4	-1	1
16	4	2	1	22	2	-2	-2
17	2	4	4	23	1	0	1
18	1	-2	-5	24	5	-1	3
		3	8			-2	9
25	5	1	2	31	5	0	3
26	2	1	4	32	3	0	2
27	6	-1	3	33	2	-1	-4
28	4	1	2	34	4	2	2
29	1	1	-2	35	1	-2	-3
30	3	1	1	36	6	-2	-1
		4	10			-3	-1
37	3	2	8	43	5	-1	1
38	6	1	-2	44	2	2	-1
39	4	-1	-2	45	4	-4	-6
40	1	3	5	46	3	5	8
41	2	3	-1	47	6	0	0
42	5	-3	-3	48	1	0	-5
		5	5			2	-3
49	6	-2	-4	55	6	1	2
50	1	8	13	56	2	-1	2
51	5	4	6	57	5	—	—
52	3	-1	1	58	4	-5	-9
53	4	3	4	59	1	-2	-7
54	2	0	0	60	3	-1	-1
		12	20			-8	-13
Totals		35	56			-5	2
		$t=2.47$	$2.60$			—	—
		$P=0.013$	$0.009$				

## APPENDIX III

## EXPERIMENT E

## SUMMARY OF DICE THROWS

*Number of appearances of die faces :*

	1	2	3	4	5	6	
Target A	103	94	107	92	98	106	
6	3	-6	7	-8	-2	6	Dev. DH
	-3	-4	15	-15	4	3	DO
Target B	90	94	113	88	105	110	
3	-10	-6	13	-12	5	10	Dev. DH
	-20	-11	25	-25	11	20	DO
Target C	110	82	104	95	114	95	
5	10	-18	4	-5	14	-5	Dev. DH
	15	-32	9	-9	32	-15	DO
Target D	113	101	94	99	99	94	
1	13	1	-6	-1	-1	-6	Dev. DH
	19	2	-5	5	-2	-19	DO
Target E	95	103	98	104	104	96	
2	-5	3	-2	4	4	-4	Dev. DH
	-1	-1	-6	6	1	1	DO
Target F	93	96	83	99	98	131	
4	-7	-4	-17	-1	-2	31	Dev. DH
	-38	-2	-16	16	2	38	DO
Totals	604	570	599	577	618	632	
Dev. MCE	4	-30	-1	-23	18	32	
Distribution of Face Frequencies							$X^2 = 4.75$ (5 d.f.)
							$P > 0.4$

## APPENDIX IV

## TESTS FOR DICE BIAS

Although spurious effects from either dice bias or the throwing peculiarities of the subject could only occur in Expt. A it was thought it might be useful to test the dice used for any appreciable irregularities.

The dice used in Expts. A, B and C were not those used in Expts. D and E although of the same manufacture. The two dice used in Expt. E were tested by G.W.F. and D.J.W. each separately making 720 throws and analyzing the distribution.

*Die-face*

	1	2	3	4	5	6	
G.W.F. Dev. MCE	12	-2	-10	-8	9	-1	
	$X^2$ (5 d.f.) = 3.28						$P > 0.5$
D.J.W.	12	-9	-10	7	-11	11	
	$X^2$ „ = 5.7						$P > 0.3$
Combined	24	-11	-20	-1	-2	10	
	$X^2$ „ = 5.0						$P > 0.3$

It will be seen that the frequency of appearance of the six die-faces is approximately equal, i.e. there is no appreciable bias. There is also very close agreement with the distribution of Dr Blundun's throws as shown at the foot of Appendix III.



## REVIEWS

FOUR MODERN GHOSTS. By Eric J. Dingwall and Trevor H. Hall. London, Gerald Duckworth, 1958. 111 pp. 15s.

The first half of the excellent introduction to this book should be read by anyone who is setting out to investigate a poltergeist case. If he is lucky enough to arrive on the scene before the phenomena have faded out, the authors suggest the kind of experimental tests that he might carry out and the nature of the precautions that he should adopt. The discussion of the four cases examined makes clear how easily an untrained investigator may miss vital points in his study of the case and in his cross-examination of witnesses. The importance of early information being sent to the Society and of quick action being taken before the local press has been brought into the case stands out clearly: this has been long recognized but it cannot be over emphasized.

From two writers who were joint authors (with Mrs Goldney) of *The Haunting of Borley Rectory* one would expect a careful and critical study of any case they are reporting and that is what they have provided. The wholly negative verdict that they give in the case of 'Harry Price and Rosalie' is what one would expect and to the writer it seems well justified. The summing up of the case of the York Museum is nicely balanced: 'an excellent example of the kind of mixture which results when a genuinely abnormal experience is not only embellished, so that it becomes more exciting and elaborate, but may also lead to developments of a pseudo-paranormal nature'.

The Runcorn case illustrates all the difficulties facing an investigator who arrives on the scene after publicity had brought crowds of investigators to the house. The most curious incident observed by Mr Stevens—a movement of a dressing-table continuing for a brief period after switching on the light—occurred when the house was full of sightseers and there were crowds in the street outside. One cannot guard properly against practical jokers in these circumstances. One useful hint for investigators is given by the authors in this case. The phenomena (save the one instance mentioned above) only occurred in the dark and in general ceased when electric torches or lights were switched on. This was done regularly only when the noise of movements in the dark was heard: someone should have switched on the light *when nothing was going on*.<sup>1</sup>

The most interesting case discussed is *The Ousedale Haunt*.

<sup>1</sup> But on this point see Mr Stevens's letter on p. 302 of this journal. ED.

Here there can be no doubt about the objective nature of the noises. They were heard in the adjoining houses and over the telephone in the police station. Here the investigators were free of publicity troubles and were well placed to examine the two main rival theories, for both of which there was much support. Mr Lambert's underground water theory fitted this case perfectly. But there was also the curious fact that the phenomena only occurred when a Miss O'Neil was living in the house, suggesting one more case in support of Podmore's 'naughty little girl' theory. There was abundant evidence that Miss O'Neil was not concerned in the production of the noises. The frustrating point in this case was that the weather changed to a very hard frost the night after Miss O'Neil left the house, and the noises abruptly ceased on the same day. The authors, however, came to the conclusion that 'beyond reasonable doubt . . . the phenomena resulted from physical and not supernormal causes'. There is one gap in the published evidence supporting this conclusion. The phenomena occurred in the winter of 1955-6. One would like to be assured that the meteorological conditions of that date have not recurred since, and that if they have, there is a good reason why the accompanying bangs and crashes have not returned also.

F. J. M. STRATTON

PSI COGNITION. K. Ramakrishna Rao. Tagore Publishing House, Tenali, 1957. xv, 179 pp. Unpriced.

When new hypotheses are expounded for explaining paranormal processes and occurrences it is often difficult for those who are brought up within the Western culture to criticise them adequately. This is because most such theories presuppose that the Western way of regarding man and the world generally is mainly true, or at least necessary, and consequently the less it is modified to accommodate the paranormal, the better.

It may well be that some Western scholars have been able to overcome both the language difficulty and what could be termed 'the reality difficulty', and have managed to understand something of Indian thought and philosophy. In *Psi Cognition* K. Ramakrishna Rao seems able to appreciate, and even to make clear to the ordinary reader, most of the more important Eastern and Western opinions concerning paranormality. He has not, however, succeeded in overcoming the language difficulty entirely. When writing of the views expressed by Indian teachers he makes use of a large number of foreign words not all of which are adequately translated. The author expresses himself in English very well, and his meaning is usually quite clear in spite of a number of

rather quaint mistakes which could very easily have been rectified before the book was printed. Frequently the definite article is omitted, there are many misprints, and often English words are wrongly employed. Thus 'Soal . . . found to his amusement very significant results' (p. 21). On p. 6 there occurs the enigmatic sentence: 'This study has also revealed that highest conviction [sic] is associated with *blocked* cases in which there was an emotionally excited intelligible mental state.' On p. 8 Mrs Willett is referred to as 'an endowed automatist' and on p. 11 there is a reference to the 'prejudiced criticism of the credulous scientist', later on (p. 33) to 'the superstitious scientist'. We are also told that H. H. Price's 'contributions to the theoretical wranglings of ESP consists of his conception of the mind' (p. 95).

In spite of these blemishes, however, this book will be welcomed by a number of thoughtful and open minded readers, because it fills a definite lacuna in the literature. The author sketches briefly, first the theories and systems of thought put forward by some of the great Eastern teachers, and in particular their attitude towards the paranormal. It is noteworthy that some of the problems appear to have been discussed pretty fully in the fifth century B.C. Sections are devoted to Yoga, Mimamsa, Vaiseshika, Nyaya, Vedanta, Buddhism and Jainism. There are summaries of, or at least references to, most of the major Western attempts to account for *psi*, ranging from Democritus to those put forward more recently by philosophers and psychical researchers. Dr Rhine in his preface does something less than justice to this work when he states that 'Mr Rao has, in this book, brought into clear review something of the heritage from his own country and the thinking of the West as it has derived from the research laboratories of parapsychology'. There are interesting discussions of Kant, Bergson and Spinoza, and a brief presentation of some of the salient points of the theories or suggestions of W. Carington, H. H. Price, Tyrrell, Hettinger, Ehrenwald, the earlier thought of Rhine, Thouless and Wiesner, Broad (reference to whose views is made frequently) Raynor Johnson, Drayton Thomas, Saltmarsh and Flew.

The author is of the opinion that 'the human mind' will not hesitate to 'adapt to the panorama of startling facts . . . if the parapsychologists could put forward an intelligible explanation of these mysterious phenomena . . .' (p. 33). However, he is dissatisfied with the attempts at theorising that he sketches, and displays considerable logical acumen in laying his finger on the weak spot of one theory after another. In particular, on page 146, he cautions against the danger of using terminology, such as 'extra-sensory



perception', which implies a theory, and commends the Thouless and Wiesner symbols.

He suggests that 'the Indian conception of the mind as a sort of refined matter perhaps facilitate the understanding of psi cognition. . . . The Advaita, Vedanta conception of the mind as something fluid and capable of taking any form which is presented to it deserves attention and development' (pp. 74-5).

The book contains numerous references, and an index. The reviewer has read it with much enjoyment and profit.

C. C. L. GREGORY

FADS AND FALLACIES IN THE NAME OF SCIENCE. By M. Gardner. London, Constable, 1957.

In ESP research, the unpredictability of subjects, the unrepeatability of experiments, the variability of effects according to time, place and experimenter, allow great scope for self-deception and greater still for sceptical criticism. The ESP workers themselves, if they are healthy, must sometimes wonder if they are as deluded as the flat earthers, the pyramidologists, the phrenologists and all the other cranks whose will-'o-the-wisps ran away with them. The protective title 'parapsychologist' avails nothing in the face of Martin Gardner, who disposes of our pretensions with much the same arguments as he dismisses the flying saucer craze and the claims for spontaneous generation. In the preface he remarks that, after the publication of his first edition, he received many irate letters from the protagonists of the different cults, approving his general approach, but objecting to his attack on their own pet theories, and to finding themselves classified with a lot of cranks. The present reviewer shares these sentiments exactly, agreeing with the scorn poured upon all the other cults, but objecting to the remarks about parapsychology.

Mr Gardner's attack on ESP and PK concentrates on Dr J. B. Rhine as the leading exponent of the cult. He shows some sneaking regard for Dr Rhine, calling him an excellent example of a borderline scientist whose work cannot be called crank, yet who is far on the outskirts of ordinary science. But he quotes some of Rhine's more extreme opinions, for instance that Lady, the 'talking' horse, had psychic powers, and that one day increased psi capacities may make war obsolete since no military secrets could be kept. Mr Gardner apparently thinks that Dr Rhine's personal beliefs have an important bearing on the evidence for ESP. He ends his chapter with a thumb-nail sketch of some other experimenters in the field, giving the impression that their work is on a par with Rhine's. But instead of names like Pratt, Stuart, Soal,

Humphrey-Nicol, one reads of Upton Sinclair's *Mental Radio* and Nander Fodor's *Haunted People*. These are not comparable with workers in the Duke Parapsychology Laboratory, and should never have been included in the chapter.

The author's material criticisms reduce themselves to a few old chestnuts that hardly deserve discussion. For instance, he suggests that so many different statistical effects count as evidence of ESP that enthusiasts will read significance into any batch of data. But no experimenter worthy of the name would embark on an analysis of a large mass of data without finding out first precisely what effects to expect and evaluate. Mr Gardner also suggests that unpublished negative work might dilute the published results to insignificance. This suggestion reveals the critic's ignorance of the magnitude of the effects reported. Mr Gardner also slips up when he laughs at us for finding evidence of concealed ESP effects in the negative results of Professor J. E. Coover's 'extensive and carefully controlled ESP tests'. In fact Coover's tests were poorly controlled; they would have been little use as evidence had the results been more striking, but they could only be quoted as apparently negative because Coover failed to apply the standard tests of statistical significance.

Other chapters of special interest to us include those on dowsing and Bridey Murphy, and with these the present reviewer has more sympathy. Mr Gardner dismisses the 'ponderous work' of Dr S. W. Tromp in a few paragraphs of which the following is a good example :

Dr Tromp admits that dowsers frequently make a bad showing when they are being tested. But this does not bother Tromp. He lists several dozen factors which may cause the failure—fatigue, lack of concentration, poor physical condition, worry, too much friction on soles of shoes, all sorts of atmospheric conditions, the presence of electric lines in the area, humidity of soil, and so on. 'Trees and their roots are particularly likely to create disturbances that prevent accurate measurements,' he writes. Although he has tried in his experiments to take all these considerations into account, it is quite clear that they are so numerous and intangible that he has a ready excuse for every dowsing failure. Nothing remotely resembling a controlled experiment is reported in the entire volume.

*The Search for Bridey Murphy* (reviewed in *S.P.R. Journal*, 38, 1956, pp. 376–8), a recent American best-seller, told how Morey Bernstein hypnotized Mrs Virginia Tighe and stimulated her to recall and dramatize a previous incarnation, a nineteenth-century Irish girl, Bridey Murphy. The matter aroused immense popular interest, and the topic has even been used as subject for a film.

Mr Gardner now describes the amusing denouement when newspaper reporters discovered the real living Bridie Corkell née Murphy. Under hypnosis Mrs Tighe had produced nothing more than jumbled recollections of incidents and personalities from her own childhood. Bridie Murphy was a close neighbour, Kevin and Uncle Plazz, characters mentioned by the spirit Bridey, proved to be childhood friends, and several incidents described by the spirit, such as receiving a whipping for scratching paint off a bedstead, had happened to Mrs Tighe herself.

The story carries a warning—not to take trance productions literally. An investigation into the personal histories of trance mediums might well yield clues to the origin of some of their spirit guides. Mr Gardner rightly pours scorn on George Devereux's 'Bridey Murphy: a Psychoanalytic View' (*Tomorrow*, Summer, 1956), an example of a learned specialist speculating on the basis of the slenderest factual evidence. But Mr Gardner goes too far in writing: 'In every case of this sort where there has been adequate checking on the subject's past, it has been found that the subject's unconscious mind was weaving together long forgotten bits of information acquired during his early years.' Has Mr Gardner never heard of Patience Worth?

D. J. WEST

DER LEBENSABLAUF DES MENSCHEN IM RAHMEN EINER WELLEN-LEHRE. By Dr Hildegard Vaubel. Justus von Liebig Verlag, Darmstadt, 1956.

In two years' time psycho-physical cosmology will be celebrating its centenary, for in 1860 Fechner published his celebrated work entitled *Elemente der Psychophysik*. He was a mathematician and physicist who 'believed that the material world had an all-pervasive psychical aspect, or 'daylight' side, into which man has some glimpses through his own conscious experience',<sup>1</sup> and his psycho-physical methods still form the basis of experimental procedure in psychology.

'Man's life in the light of a wave theory' is an attempt to give a structure to the psycho-physical problem (das psychophysische Problem zu unterbauen) by means of a theory of waves. Dr Hildegard Vaubel follows in the psycho-physical tradition of researchers who collect data from many disciplines in an endeavour to provide a unitary theory which will account for the world as we know it. She writes as a psychiatrist who is dissatisfied with the conventional basis of medicine, and believes that the data of

<sup>1</sup> R. S. Woodworth, *Experimental Psychology*, New York: Holt, p. 392



psychical research must be taken into account when formulating a new unitary framework for science.

She takes the view that physical as well as biological and spiritual phenomena are only the secondary outcome of something more basic which she calls *waves*. These waves, then, although the concept is borrowed analogically from physics, are not identical with the waves of the physicist. They are thought of as bridging discontinuity in the organic world just as physicists invoke electromagnetic waves as hypothetical structures for bridging physical discontinuities.

It is perhaps not realised widely enough that biology has by no means succeeded in bridging the psycho-physical gap by means of central nervous structures. Dr Vaubel points out many of the biological data which defy exhaustive explanation in terms of mechanisms. She mentions the sea urchin egg experiments of Driesch where twinning was induced by mechanical division of the eggs. Why should two perfect, though smaller individuals result? If an earthworm is cut up and the pieces are sewn together, why should the patched creature behave as a whole? Indeed, why should half an earthworm grow the other half? Why should a person be the same after convulsions due to electric shock treatment, epileptic fits or, for that matter, sleep? What preserves memories for so long and so minutely? Dr Vaubel mentions some of the neurological findings which indicate that the brain's function is that of facilitating rather than that of storing memories, such as the mass action of, and the imprecise localization of function in, the cerebral cortex. She treats the data of psychical research as biological continuities in spite of space and time gaps. If something is known to one person, in one place, at one time, how does another, at another time and elsewhere acquire the information? She considers this as a problem allied to, though not identical with, the question why one and the same person is informed concerning his own doings at other times and in other places. Personal survival is regarded as indicated primarily by the facts of biological continuity in the face of physical and temporal discontinuity, and only secondarily by the data of psychical research.

She considers that the generally recognized integrative biological hierarchical structure is continued 'upwards' so that there is a superordinated level beyond the conscious individual, called the mother-sphere. This mother-sphere contains the monads which compose the individual and these stable monad-combinations can, in their turn, form mass souls, frequently of a transitory nature, which case this new group forms one organism.

The personality content of each individual is the sum of his conscious and his unconscious mind, this latter being the functionally superordinated residuary person (*Restpersönlichkeit*). As a man matures, so his superpersonal centres yield up to the organism more and more 'personality parts' (*Persönlichkeitsanteile*) which are withdrawn once more into the mother-sphere when he is asleep, unconscious or declining in old age.

The waves are the ubiquitous links which unite discontinuities *via* the higher levels in the mother-sphere, so that a person  $A$  in place  $p_1$  at time  $t_1$  is linked with  $A$  in place  $p_2$  at time  $t_2$  by means of waves from his higher self. The same arrangement is employed to explain extra-sensory communication, instinctive patterns in plants and animals, personal memory and identity and psychopathological disturbances. The higher level is thought of as being in a different and more comprehensive time dimension  $\tau$  as opposed to the time  $t$  which contains the physical going on of things. Everything exists in a psychic aether or spiritual space (*geistiger Raum*), which provides the projective plane for all events past, present and near-future, and is the ultimate source of all creative and emotional energy, as well as the ground of all reasoning and intuition.

The organism is thought of as continuously self-recreated and self-rematerialized by the higher person, though in another sense it is supposed to have an independent existence when the incarnate spirit is temporarily withdrawn. The higher residuary person thus has materializing powers and may create for itself organs and even doubles of 'fine matter' which may have different degrees of density, ranging from the wholly invisible to the plainly visible form, and which are invoked to account for paranormal physical phenomena, out of the body experiences and veridical hallucinations.

The system is not, as yet, worked out in any detail. For instance, it is not quite clear how there could be such perfectly complete conservation of individual personality content when the superpersonal parts of the individual are themselves thought of as communicating with one another by way of waves.

Dr Vaubel has evidently given considerable thought to this work. Living in a country where an organicist view of the world is widely held, such constructions probably come more easily than they would in this country or in America, where atomistic empiricism has for so long been the metaphysical favourite. Perhaps Fechner's dream will only be realized when the scientific background of basic assumptions permits a combined atomistic and organicist structure of the world.

ANITA KOHSEN

IST DIE WÜNSCHELRUTE EIN ABERGLAUBE? By C. Graf von Klinckowstroem. *Erfahrungsheilkunde* (1958) Vol. VII, May, pp. 211-19.

Until recently Graf von Klinckowstroem was a member of the German Society for the Prevention of Superstition (*Deutsche Gesellschaft Schutz vor Aberglauben*). It seems that his resignation was preceded by a somewhat stormy and explicit exchange of letters between himself and the Chairman of the Society in question, Herr Brunner.

In an article entitled 'Ist die Wünschelrute ein Aberglaube?' (Is the divining rod a superstition?) K. gives some indications of the contents of this correspondence in which B. appears to have expressed the view that only a superstitious clot (ein abergläubiger Tropf) could possibly believe that the dowsing reaction is due to 'external stimuli', an opinion clearly not shared by K. The German S.P.S. has apparently set itself the task, applauded by K., of fighting modern beliefs in witchcraft, occult swindlers, clairvoyant mediums and astrologers, and has now decided to add dowzers.

K. believes that the dowsing reaction is an *allergic* phenomenon due to a 'geopathic' constitution. He holds that, although we are surely entitled to the conclusion that the genuine dowser responds to stimuli emanating from the soil, the dowser's weak spot is his own interpretation of his reactions, and that a geological expert is required to supply this.

In support he cites the story of how a special committee of the town of Marienberg appointed four dowzers to locate a suitable place for drilling in order to find a long lost spring, whose waters were once thought to have healing properties. The four, wholly independently, indicated the very same spot '*with truly conspicuous coincidence*', (K.'s italics), differing only as regards the depth to which it would be necessary to drill, ranging from 60 to 180 m. It remains to be added that, even at a depth of 200 m., not a trace of moisture was to be found although there were some trifling indications of ore and precious metal.

It is difficult to see at what stage a geologist would have been of assistance.

ANITA KOHSEN

LA TÉLÉPATHIE (*Bilan du Mystère*, Series 5). By Robert Amadou, Paris, Grasset, 1958. 155 pp.

This interesting little book, written in a popular style, describes the main facts and theories concerning Telepathy. After a brief historical introduction to the subject comes a most informative



account of the methods of tricksters in obtaining pseudo-telepathy either before theatre audiences or in the laboratory. The author also gives in some detail an account of two classic cases where distinguished persons have deluded themselves into an entirely unjustified belief in paranormal phenomena.

The remainder of the book is devoted chiefly to a consideration of the work done in America and in England by Rhine, Soal and others and ends with a general discussion on the nature and future of Telepathy.

Unfortunately the illustrations are not up to the standard of the rest of the book. Some have no descriptions whatever and their purpose is by no means clear. With others the captions are wrongly attributed; for instance the Zener cards used by Rhine and the Animal cards used by Soal are transposed. These are mere slips. A more serious criticism must be made of the series of illustrations showing an object looked at by an Agent (not identified) and the corresponding picture drawn by a Percipient (also not identified). In some cases the resemblance is striking but in the absence of any indication as to the number of hits and the total numbers of guesses there is clearly no evidential value in these results.

Another plate shows (again with the wrong sub-title) one of the best scoring sheets, 23/25 and 20/25, obtained by Dr Soal and the reviewer with the Welsh boys. It would have gratified both experimenters if their permission had been obtained for the publication of this picture. This lapse, however, does not detract from the value of the book as a general guide to the present position of this subject.

H. T. BOWDEN

BULLETIN OF THE SOC. ITALIANA DI PARAPSICOLOGIA. 2nd half of 1957.

This issue was delayed until the Spring of 1958 so as to include an account of a conference in Florence in January 1958. A conference had already been held there in May '57 expressly to consider 'the scientific method and its application to psychic research'. This meeting had been so fruitful that a referendum on the subject had been sent to various learned men (members and non-members) and their replies were to be considered in the second conference. Replies were received from some 20 scientists and the above issue of some 50 pages is devoted entirely to recording their various opinions.

It is not possible in the space available to give more than a brief resumé of some of the opinions expressed. Prof. F. Egidi, the

President of the Society, opened the meeting by summarizing the replies and drawing the conclusion that all opinions should be welcomed provided that they fulfilled certain indispensable conditions—'seriousness, exactitude, critical severity, level-headedness and the exclusion of every prepossession and every interest apart from that which spurs all scientists towards the pursuit of truth'.

Dr E. Marabini of Bologna, claimed that there was only one scientific method which had been the same from Aristotle to Einstein—careful observation and logical reasoning, followed where possible by experiment. Dr R. Assagioli, President of the Florentine group, insisted that they must follow the canons of Francis Bacon, the founder of the scientific method, but must not forget the pragmatic principle that whatever produced an effect was real and that, as parapsychology dealt with effects which were not repeatable at will and were not measurable, it became all the more important to reason clearly. Prof. Wm Mackenzie, the Honorary President of the S.I.P., made the point that, in spite of the enormous progress of technology, the vaunted scientific method had arrived at conclusions which had constantly to be revised and that it was possible to trust it too much: parapsychology was a branch of psycho-biology and too much insistence on scientific method might stultify the result; for example, it might provoke inhibitions in a 'sensitive' and prevent the appearance of the awaited phenomena. Such a negative attitude could not truly be called scientific. While aiming at complete objectivity one must not assume that there was only one scientific method. Dr F. Recanelli, a medical healer, stated that in psychic matters there were far too many imponderables for one to be sure exactly what the scientific method was: certain transmissions of energy occurred which orthodox science could not explain. Prof. G. Piccardi maintained that the evolution of organisms had proceeded ever to new and more complex forms and that science was ever discovering new and unsuspected facts. He agreed with Prof. Needham of Cambridge that scientists must not flinch before the challenge of these new discoveries, knowing that they did not know. The phenomena of parapsychology, like many other phenomena in nature, were sporadic and there had not yet been discovered a valid method of dealing with them. Objectivity and patience were needed and perhaps in time the very phenomena themselves might suggest the method to be followed.

In spite of the many conflicting opinions, at the termination of the Conference a resolution was drawn up which was unanimously adopted—briefly, that the phenomena existed and should be in-

vestigated with all the technical means available and by the method common to all the sciences and, where possible, by experimentation.

The following issue of the above publication, for the 1st half of 1958, contains a most commendable summary of all the reviews of psychic research for the past year from different parts of the world.

M. T. HINDSON

THE JOURNAL OF PARAPSYCHOLOGY, XXI, No. 4, December 1957, Duke University Press, Durham N.C.

The Editor asks why national defence overlooks Parapsychology. He suggests that it should be interested for two reasons: because its findings challenge the theoretical basis of Communist materialism and because ESP might be a useful source of military intelligence. The first point is, I think, doubtful. On the second, would not all representatives of the services say that merely probable information is without usefulness? It may be suggested that if parapsychologists want their science to contribute to defence purposes they should concentrate research on the problem of getting as close an approximation as possible to 100 per cent accuracy of information.

Dr Kooy, who is a Professor of theoretical physics in Holland, discusses the implications of the relativity view of space-time for the understanding of pre-cognition, in an article: 'Space, Time, and Consciousness'.

G. L. Magnan reports a further stage of his study of a single subject showing forward displacement on to the card ahead of the target card. He used a dual target method with 5 shapes of flowers and 5 colours so that the chance of being right was 1 in 25. Highly significant successes were found both on the predicted forward displacement and also directly on the target. The tendency seemed to be that the hits were both on colour and form, and not on these two aspects independently.

S. R. Binski reports two exploratory tests of PK undertaken at the University of Bonn with coins and a roulette board. No significant results were obtained with 116 of the subjects tested, but one subject showed highly significant results in both tests. He seems to have been a genuinely gifted PK subject. Some methodological crudities in the experimental work are unfortunate, but are insufficient reason for rejecting the result. It is better to design an experiment to exclude precognition as a factor than to leave it as a possibility and to argue against it afterwards, particularly when the argument depends on figures that are not published.

There are reviews of the Rhine and Pratt book on *Parapsy-*



chology, of the Parapsychology Foundation *Report on four conferences*, and of Dr West's book on *Eleven Lourdes Miracles*.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

New York, A.S.P.R. (Vol. 52, No. 1, January 1958. \$1.50)

In 'Physical Phenomena in Psychical Research', Professor C. J. Ducasse, following C. D. Broad, begins by pointing out that physical phenomena purport to contravene one of the 'basic limiting principles' of all normal thought and action. They are comparatively neglected because of the history of concomitant fraud, the paucity of good physical mediums, and the difficulty of repeatability. Various categories of physical phenomena are reviewed and D. D. Home is given prominence.

Professor Ducasse's summing up is a model of fairness. Were the phenomena reported genuinely paranormal? No answer can be given. The cases are seldom adequately reported and our knowledge of the actual circumstances, of psychology, of hallucinations, and of the limits of fraud is insufficient to enable a judgement to be made. At the same time, Ducasse argues, allegations of fraud, malobservation, or misinterpretation, 'have to be scrutinized *as closely and as critically* as must be the testimony for the reality of the phenomena.'

In the same issue J. Fraser Nicol and Betty H. Nicol investigate a case of a curious 'hunch'. In 1955 a Boston welder had a premonition that something might be wrong on another site at the other side of the town where a colleague and friend was working in a trench. He drove over just in time to save his life. The account makes very good reading and it is no surprise to find that the investigators have asked all the right questions—for example, as to the frequency of accidents in this type of work.

W. H. W. Sabine reviews *The Ghosts of Versailles* by Lucille Iremonger, and Dr Montague Ullman reviews *From Medicine Man to Freud*, an anthology edited by Jan Ehrenwald, M.D. Mrs Lydia W. Allison contributes a brief history of the American Society for Psychical Research.

(VOL. 52, No. 2, APRIL 1958.)

In yet another of her consistently thorough investigations Dr Gertrude R. Schmeidler studies the Agent-Percipient relationship. 'What does the agent do, in 'sending' material telepathically, that changes what the percipient could have done in reaching this material alone, clairvoyantly?' I give below the greater part of Dr Schmeidler's summary of the experiment and its conclusions :

Percipients were led to believe that all eight runs of an experiment in ESP would be of the GESP type. In fact, only two pairs of runs were of the GESP type, and the other two pairs were of the clairvoyant type. In one pair of clairvoyant-type runs (the 'success' runs) the agent was instructed to hope (without knowledge of the target) that the percipient would succeed; in the other pair (the 'failure' runs) the agent was instructed to hope (without knowledge of the target) that the percipient would fail.

There was a negative correlation between total GESP scores and 'failure' scores; and a low positive correlation between the two pairs of GESP scores. The difference between these correlations was significant at the level of  $P=0.013$ . This is interpreted to mean that there is an effective agent-percipient relationship in ESP.

There was a low positive correlation between GESP scores and 'success' scores. This was suggestively different from the negative correlation between GESP and 'failure' scores ( $P=0.064$ ). This is interpreted to suggest that a part of the agent's role is the general facilitation or inhibition of the percipient's response, apart from transmission of target content.

For subjects who were predicted [by Rorschach test and interview] to have low GESP scores, GESP scores were significantly lower than mean chance expectation ( $P=0.0014$ ) and also significantly lower than 'success' scores ( $P=0.015$ ). This is interpreted to suggest that the agent can transmit target content to the percipient.

Dr Jule Eisenbud reviews *Modern Miraculous Cures* by Dr François Leuret and Dr Henri Bon, and also *Eleven Lourdes Miracles* by Dr D. J. West. Dr Montague Ullman reviews a new edition of *Hypnotism* by George H. Estabrooks, and there is an unsigned review of *Sense and Nonsense in Psychology* by H. J. Eysenck.

DENYS PARSONS

## CORRESPONDENCE

SIR,—It has been a pleasure to read *Four Modern Ghosts* so ably written by Dr E. J. Dingwall and Trevor H. Hall, and my grateful thanks are due to them for sending me a copy.

Their purpose to establish facts in order to arrive at the truth is the desire of all true researchers. As I was present and witnessed some of the disturbances I feel it my duty to make a few comments and corrections for truth's sake.

It is stated (p. 82) that, 'No-one seems to have flashed on a light *when nothing was going on.*' (Their italics.) From this the conclusion is drawn that ample opportunity was provided for fraud. This is far from correct. Lights constantly swept the room from

the torches of the watchers, and the main light was frequently switched on even to the annoyance of some who were anxious to witness the alleged disturbances. Most of those present were sceptical and had never believed such things could be possible. It is not the natural behaviour of such people to sit passively in the dark thereby giving opportunity for some trickster to deceive them. I regret the reports did not make this clear, the reason being that when nothing happens there is nothing to report.

Regarding the jig-saw incident (p. 83) we are told, 'Thus, had Mr Stevens when arranging the jig-saw puzzle at the back of the dressing-table, moved quietly to the side of the table and kept his hand near to or preferably touching the puzzle, he might have discovered what moved it if it did move.'

But that is just what I did which I thought I had made abundantly clear in conversation. On this particular occasion I, myself, replaced the contents on the dressing-table which had been sent across the room a few moments before. The jig-saw attracted my attention for I expected that any movement of the box would be accompanied by a rattle which would be a warning signal for me to act. I had pulled the dressing-table to the bottom of the bed against the wall and placed the jig-saw in the corner nearest the wall close against me. In front I stacked two books. I then took my stand at the bottom of the bed about two paces away. No-one was near the dressing-table. Immediately the light was switched off, I silently slipped over to the table close to the jig-saw, a matter of inches. One hand was touching the wall and my other arm was extended so that nobody could come near. Dead silence prevailed and I fail to see how anyone could possibly have thrown that box except myself, and I certainly did not. Articles were thrown one by one. I heard the two books go. Then came the rattle of the jig-saw. Immediately I flashed on my torch. The box was rising quickly in the air towards the bed, not far from the ceiling, in full view of all present. There is no question of 'if it did move'. It moved all right.

I cannot see how anyone could have given 'a sharp push' (p. 83) to the dressing-table to make it rock at my request. My torch shone at the beginning of the third shake and almost simultaneously someone switched on the light. It seems to me that a hoaxer would have been caught in the act and a quick movement by anyone would have been heard and observed. The article of furniture was fairly heavy and when I tried to rock it I found it firmly planted on the floor.

The dressing-table did not rock in the normal way. It shook and shuddered with a loud rattling noise. It seemed to dance on



the floor. No-one was near it at the time. This amazed me considerably.

Looking back I still cannot understand how the dressing-table was moved so far along the floor. When it was pushed against the wall it was too far to move from the bed. Had one stepped out of the bed the movement would undoubtedly have been detected. We were close against the bed with our ears on the alert and our torches at the ready. I made sure that no one was under the bed. I came to the conclusion that the family was genuinely distressed and at times frightened.

The question as to why the occupants did not ensure sleep by turning on the light (p. 38) relates to the circumstances of that disturbed household. When Press reporters and investigators are there every night it becomes extremely awkward and embarrassing to refuse them admittance. Had they done so it is probable that suspicion would have been aroused that they had something to hide, or that the whole thing was after all a fake. Besides if these strange disturbances were genuine it is unlikely that the family would be content to sleep with the light on as though nothing had happened. Their perturbed state of mind would not rest until the mystery was solved or the trouble had ceased. It is so easy to misunderstand conditions prevailing inside the house when viewed from the outside. I understand, however, that when they were left alone they did sleep with the light on.

The case of the late Miss Whittle might be a matter of her particular temperament. She, apparently, wished to have nothing to do with such a subject. So far as I am aware she did not express disbelief and I doubt whether she could have added much information. It is unfortunate we cannot have her testimony.

Mr Sam Jones has now left his old home in Byron Street. A recent report in the *Liverpool Echo* (June 6th, 1958) states, 'When the trouble began Sam Jones weighed a sturdy 15 stones. To-day, he is down to nine stones. Neighbours told me 'Sam had to leave that house. It was killing him.'

The authors are sufficiently well-informed and wise enough to suspend judgment rather than deny the possibility of poltergeist phenomena. For my part I am reluctantly forced to accept the poltergeist at Byron Street as a fact, leaving such problems for further investigation.

W. H. STEVENS

SIR,—In the preface of *Four Modern Ghosts* the authors, Eric J. Dingwall and Trevor H. Hall, state the book is intended to 'assist students to evaluate material submitted to them for scrutiny'.

I suggest that, in certain particulars, it is an object lesson of a method to be avoided if truth is the objective.

Of the four sections of the book I am mainly concerned with the one dealing with Harry Price and an alleged materialization of a child called Rosalie. The authors' hypothesis is that the whole episode from beginning to end was concocted by Price for the sake of publicity and the séance in question never took place at all. They state that one objection to this hypothesis rests on my testimony, which they consider so important that they proceed to dissect and examine it in detail.

I have known Dr Dingwall well for some 30 years. Mr Hall collaborated with us in our long report, *The Haunting of Borley Rectory* (*Proceedings*, 51, 1955) and over a long period he and I have had a voluminous correspondence. Mr Hall was a newcomer to psychical research and eager to learn as much as he could. In 1954 he wrote asking me to 'jot down details of any private information' I had about the Rosalie case (note his use of the word 'private'). I did so willingly, describing my notes as 'my impressions from memory of the occasion [on which] Price told me of it.'

It is this testimony of which the authors make free use. Neither of them told me they intended to use it, nor asked me whether I would care to amend the wording of a hastily written account to avoid any misunderstandings of my position and make it more suitable for wider publicity. I am on the telephone; I can write; Dr Dingwall is frequently in London where I live. But I knew nothing about the book until I was rung up by some newspapers asking me for comments on the use made of my name!

I submit that when publishing material privately supplied by a colleague, permission should first be asked and then the draft shown to the witness to see if he or she has any useful comments to add. That is not only a very elementary courtesy but is an essential requisite preceding any impartial judgment. No historians of good repute would act otherwise. As it is, the authors have expressed gratuitous suppositions regarding my views which are entirely misleading. I quote a few examples from their book with my comments:

1. (p. 55) '[K.M.G.] found Price deeply disturbed, almost distraught' and 'shaken to the core by his experience.'

This was not what I told Mr Hall. The words are lifted from Dr Tabori's book about Price, describing a conversation he had had with an un-named friend. I submit that a descriptive sentence in the third person by another author should not be quoted in inverted commas as first-hand testimony.

2. The authors add: '[K.M.G.] evidently accepted the story which Price proceeded to tell'.

Not an accurate description.

3. 'That Mrs Goldney still believed him as late as 1949 seems to us to be demonstrated beyond reasonable doubt.'

As it is not accurate to say I 'believed' Price at any time it follows I could not be 'still' believing the story in 1949.

4. 'One can only conclude that . . . Mrs Goldney . . . had no doubts whatever in December 1937 regarding Price's absolute integrity and would accept the story he told her, however incredible, as true.'

A gross distortion.

5. (p. 57) 'All the more important and scientific members of Price's Council at once resigned, with the exception of Mrs Goldney . . . '.

True, but not the whole truth. It would have conveyed a truer picture if the authors had added that I told them I decided not to resign because it was suggested to me by colleagues that someone should remain in close contact with Price in order to know what was going on in his laboratory. I did not know at that time as much about him as I have learned since his death, but I made then no secret of my adverse opinion of some of his views and activities. In spite of this I was able to remain friendly with him until his death, as is mentioned. This, however, is a very different picture to that suggested by the authors' inuendoes, such as on p. 57: 'There is reason to suppose, however, remarkable as it may seem, that between 1933 and Price's death in 1948, Mrs Goldney's faith in him was completely if temporarily restored'.

Besides myself Mr Price's secretary, Miss Beenham (who was working with him at the time he stated he saw the Rosalie materialization) might have been considered a witness worth contacting. The authors did not attempt to get into touch with her, though I could have supplied her address. I submit all first-hand witnesses of importance should be contacted where this is easily done, and their testimony quoted, even if what they are likely to say may not fit in with the authors' hypotheses.

The authors' and my long, detailed report, already mentioned, on Price's major activity at Borley Rectory, should have given sufficient insight into his character and methods to render superfluous further elaborate examination of his activities, particularly when, as in the case of Rosalie, the examination is based on so many surmises, suppositions, presumptions, assumptions, etc.



I might add that I have recently come across correspondence showing that in 1947 Dr Dingwall, from across the Atlantic, was similarly suggesting that a certain subject, with whom an American investigator stated he had conducted experiments, had 'never existed' and that the experiments, though reported in a well-known Journal, were 'never done'.

I further note that on pp. 56-7 the authors describe Price's investigation of Rudi Schneider and again make use of private information without first seeking acquiescence either from me or from others primarily concerned. If my experience at their hands is a fair sample of their methods of dealing with witnesses, one cannot but have doubts as to the value of any conclusions they may have reached.

K. M. GOLDNEY

SIR,—May I draw the attention of members of the Society to a recent paper by Michael Polanyi *Zeitschrift für Physikalische Chemie*, 15, 1958, p. 290) which, it seems to me, may have important implications for statistical experiments in general, and PK experiments in particular. Briefly Polanyi considers a model in which microscopic 'coins' are subjected to the random bombardment of gas molecules; (the bombardment being the result of the thermal agitation of the molecules). If the 'coins' are all biased for 'heads', at low temperatures they will all settle down 'heads' at equilibrium; whilst at a sufficiently high temperature the thermal agitation will more than offset the bias, and at equilibrium 'heads' and 'tails' will be equally represented. At intermediate temperatures the equilibrium distribution will lie between these two extremes. From this Professor Polanyi concludes that the probability distribution of a biased system is indeterminate, because it depends upon the *intensity* of the randomizing process. Polanyi considers the implications of this in various fields (e.g. genetics): but it seems to me to raise difficulties in connection with PK research. Thus if both the random shaking of dice and the target selection (e.g. from random number tables) are subject to this hyper-regularity, then spurious statistical effects will arise.

MICHAEL COLEMAN

## NOTICES

THE Library will be closed for stocktaking from 1 October to 14 October inclusive. Will members who have books on loan please return to the Secretary by 30 September at the latest.

THE Thirteenth Frederic W. H. Myers's Memorial Lecture by

Professor C. D. Broad, D.Litt., F.B.A., entitled *Personal Identity and Survival* was published in July 1958. Copies may be obtained from the Secretary price 2s. 2d. post-free. (Members 1s. 8d.)

A SOCIETY Member, Mr P. R. Lawton, a teacher of Physics, proposes to collect a small group to co-operate in planning spare time experiments of an analytical and statistical nature on lines similar to previous card-guessing experiments, but adopting other lines of approach. The work requires participants with some understanding of statistical procedure and preferably with an acquaintance with one or other branch of physical or biological science. It would probably occupy one evening per fortnight and would be more convenient for members residing in Central or South London areas. Anyone interested is invited to contact Mr Lawton at 59 Galpins Road, Thornton Heath, Surrey, or to advise Mr J. H. Cutten through the Secretary.

PRINTED IN GREAT BRITAIN  
BY ROBERT MACLEHOSE AND CO. LTD  
THE UNIVERSITY PRESS, GLASGOW

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